

The paragraphs are as follows :—

Mr. Baldwin Latham, in a discussion which ensued upon the Report of the Committee on Decrease of Water Supply (*Quarterly Journal Roy. Met. Soc.*, p. 223), said :—

"The records showed that there appeared to be a recurrence of low water every ten years. There was lower water in 1824 and in 1835; the period 1844-5 was low, especially when compared with the years immediately before and following; 1854 was remarkably low; also 1864-5, 1874-5, and now they come to the present low period of 1884-5.

"As to what was the cause of this marked periodicity it was very desirable to ascertain, and, having pointed it out, probably some light might be thrown on the subject."

The other is from the *American Meteorological Journal* under the heading "Cold Winters in Michigan," and the writer says :—

"It is interesting in this connection to notice that the local reports of extremely cold winters place them at intervals of between ten and eleven years. . . . The winter of 1842-3 is thus shown to have been extremely cold; also the winter of 1853-4; the winter of 1863-4 noted for its terribly cold new year; the winter of 1874-5, when the frost penetrated into the ground in Port Huron four to six feet, there being scarcely a thaw between January 1 and the middle of March; and, lastly, the winter of 1884-5, which beats the record for extreme cold during January and February."

I may add that before I had seen either of these paragraphs I had concluded from other sources that the years 1821-2-3-4, 1833-4, 1844-5, 1866-7, and 1875-7 were characterised by mild winters in Europe and unusual cold in Iceland and America, being preceded in most cases by drought during the summers; but of course this represents merely the result of a preliminary glance at some general records of noteworthy seasons.

November 9

E. DOUGLAS ARCHIBALD

Photography of the Corona

I HAVE been following with interest the communications which have been made from time to time to *Science* by Mr. W. H. Pickering regarding the photography of the corona in full sunshine. Whilst admiring the manner in which he has built up his theoretical objections to its possibility, I am forced to dissent from his deduction from the fact that the theory does not fit in with the results actually obtained during the eclipses observed in Egypt and the Caroline Islands. I have in my hands at present spectrum and other photographs of the corona made during the expeditions to those localities, and from them I gather he has evidently much underestimated the photographic brightness of the corona as compared with that of the sky. As I propose shortly to read a paper before the Royal Society on the subject, I cannot enter into details at the present moment. All I will say is that the comparative photographic intensity of both can be estimated with approximate exactness from the data I have by me.

I write this for insertion in your columns, as in your last issue you have a note regarding Mr. W. H. Pickering's communications on this subject.

W. DE W. ABNEY

Permanence of Continents and Oceans

MANY naturalists are accustomed, in lecturing, to speak of the existing ocean basins as "permanent." Though this must to a large extent be a true statement, many geologists at all events must be perfectly aware that the former distribution of life requires that nearly all land, however remote at present, must have been, perhaps more than once, in connection with each other. Tropical South America is perhaps the most isolated continental province now existing. I would ask these naturalists to explain how its species of tropical genera not peculiar to it got there, and how many of them came to be represented in Europe in Tertiary times.

That the lands are always chiefly centred about the same spots, and also the converse, would, I think, be an acceptable way of putting it; but that the Atlantic was never bridged except towards the Arctic and Antarctic circles, is a statement that is unwarrantable because contradicted by unimpeachable evidence.

J. STARKIE GARDNER

History of Elasticity

IN order to estimate Poncelet's services to the theory of elasticity I am extremely desirous of examining certain works by

him. These works are not to be found in the London or Cambridge Libraries, and the Paris booksellers to whom I have applied despair of being able to procure copies. It will hardly be possible for me to go to Metz to examine them before the publication of the first volume of the "History of the Mathematical Theories of Elasticity." Possibly some of your readers may know of the existence of accessible copies in this country. If so, I should esteem it a great favour if they would communicate with me at University College.

In 1827-29 Poncelet gave at Metz a "Cours de Mécanique Industrielle aux Artistes et Ouvriers Messins." In this "Cours" various important points of theoretical elasticity were considered for the first time.

It was published as follows :—

(a) Part I. Lithographed edition, Metz, 1827.

(b) Part II. First edition lithographed 1828; second edition lithographed 1831.

(c) Part III. Lithographed edition, 1831.

(d) Part I. First printed edition, Metz, 1829; second printed edition, Metz, 1831.

It is needful to remind your readers that there are numerous other works entitled "Mécanique Industrielle," by Poncelet, published at Liège, Paris, and Brussels, differing from each other, and entirely from the above. These I have examined, but they do not contain what I require.

KARL PEARSON

University College, London, November 15

The Heights of Clouds

In the very favourable notice of our "Mesures des Hauteurs et des Mouvements des Nuages," in *NATURE* of October 29 (p. 630), there exists a misunderstanding as to the probable errors of our measurements, which makes our observations seem much more inexact than they really are. I therefore ask your permission to correct it.

Mr. W. de W. A. says: "Perhaps one of the most easily-observed clouds is the cumulus, and we find from a table given that the *probable error of observation* is very considerable." But, in fact, what is there referred to as an error of observation is not such an error; it is the probable uncertainty ("incertitude probable") *depending on the variability of the phenomenon itself*.¹ This is expressly stated in the treatise. On p. 39 (that following the table quoted) there may be read: "L'incertitude calculée comprend ainsi et celle dépendant de la variation des hauteurs des nuages, et celle provenant des erreurs d'observation. Celle-ci est cependant assez petite par rapport à la première et à peu près constante pour les différentes heures du jour, comme on le trouvera en la calculant séparément à l'aide des erreurs moyennes *m*." That mean error *m* is just the *mean error of observation* in the height of a cloud, and in our "list of observations" we have given it for every observation, as well as the corresponding mean angular error of the observation. By calculating the probable error of an observation of cumulus by means of those values of mean errors we have found it to be 35 metres (instead of 748 metres, as Mr. W. de W. A. thinks it to be), and the probable error of the mean is found to be 3 metres (instead of 40 metres), the whole number of observations being 134.²

Thus the above assertion is fully justified, viz. that the errors of observation may be quite neglected when compared with the uncertainty depending upon the variability of the heights of the cumuli from one cloud to another. That variability is itself a phenomenon worthy of investigation, varying as it does according to the hour of the day and the barometrical state of the weather, and that is the reason for which we have calculated it. As to the mean angular error in observing a cloud, we have found it very often to be inferior to that obtained in observing the centre of the sun, and in all the better observations that error is fully comparable to the error in observing the sun, as may be seen from our treatise. This proves that, for such observations, the uncertainty of an identical point of cloud did not exist at all, the whole uncertainty depending on the unavoidable instrumental errors. Still it may be that the errors are

¹ For the figures in the table quoted represent simply the *probable difference* of an observation (of the mean found) from the *true* mean calculated by the method of least squares.

² For the higher clouds, as the pure cirri, this error was often very great indeed, but it was so because their distance was much too great when compared with our basis, the parallax obtained being not greater than 1° or 2°. This year (1885) the measurements are regularly pursued from the ends of a basis of 1302 metres, and we can now determine with great accuracy the height even of the most elevated cirri, as well as their horizontal and vertical velocities.

somewhat less in using a photographic theodolite than in using our instruments. But on the other hand our method enables us to observe the clouds even in twilight and moonlight, in rain and storm. Also, it is, no doubt, much cheaper than the photographic one.

N. EKHOLM

Up ala, November 6

The Helm Wind

SOME years ago I passed a summer at Melmerby, which is about the best place for seeing the "helm," which is incorrectly described as affecting the Penrith valley (for, in fact, it never extends to Penrith) by your correspondent, M. Woeikoff.

Melmerby is at the foot of the Cross Fell range, and gets the "helm" with great violence. When an easterly wind comes on, the summit of Cross Fell becomes clouded; it *puls on its helm*: then from this a violent cold wind pours down the hill-side (which is steep) and rises up again at no great distance. At Melmerby, and places similarly situated, there is clear sky, for the moisture in the sky is invisible, but further from the range it is precipitated where the current rises, and there is cloudy sky, without the strong wind. The phenomenon is, in fact, precisely that at Table Mountain, where the cloud on the crest is called the "table-cloth."

Judging from M. Woeikoff's description there seems to be a difference in the phenomena. Probably owing to the *gentle slopes* of the Varada chain the air does not seem to *rise again*, and there is no cloud-bank parallel to the chain. It would seem, too, that the wind extends to the *west*, unless there is a misprint.

J. F. TENNANT

37, Hamilton Road, Ealing, W., November 13

THE MODE OF ADMISSION INTO THE ROYAL SOCIETY

OUR contemporary *Science*, in the last number which has reached this country, makes some remarks concerning the admission of candidates into the Royal Society, against which, in the interests of truth and accuracy, it is our duty to protest, the more especially as it is also implied that the French system of canvassing those who are already Fellows of the Society is also adopted.

The statements actually made are (1) that there is an "actual competitive examination, on the result of which a certain number of successful candidates are annually chosen," and (2) "that the English method has the additional disadvantage that it does not secure the men whom it is most desirable to honour." We read also, "During the schoolboy period the distinction between different individuals is a distinction of learning, and an examination is not unfitted to discover the boy who deserves reward. But learning is not the quality which a State needs to make it great. Casaubons are not the kind of men who have built up English science. The qualities which ought to be encouraged, and which it should be a nation's delight to honour are qualities too subtle to be detected by a competitive examination."

For the benefit of our transatlantic brethren we may as well state the facts as we know them. For reasons into which we need not enter here, as they do not affect the question at issue, nearly forty years ago the Royal Society determined to limit the yearly admissions to fifteen; and to throw upon the Council the responsibility of selecting the fifteen who are to be nominated for election, a general meeting of the Society reserving to itself the right of confirming or rejecting such nomination. It may be instructive to remark that for thirty years that right has not been exercised.

The way in which the matter is worked is as follows:—The friends of a man, who are already in the Society, and who think he is entitled to the coveted distinction, prepare a statement of his services to science, in many cases without consulting him in any way. This paper thus prepared is sent round to other Fellows of the Society, who are acquainted with the work of the candi-

date, and who sign it as a testimony that they think he is worthy of election. In this way when the proper time arrives some fifty or sixty papers are sent in to the Council for their consideration. In the Council itself we may assume that the selection of the fifteen is made as carefully as possible in view not merely of individual claims but of the due representation of the different branches of science. It is not for us to state the safeguards or mode of procedure adopted, but we think we may say that the slightest action or appeal to any member by the candidate himself would be absolutely fatal to his election. Finally, we may say that, years back, when a heavy entrance fee had to be paid, there were cases in which the question had to be put to one whose friends were anxious to see him elected, whether he would accept election. The small yearly subscription of 3*l.*, now the only sum payable, makes even this question unnecessary at the present time.

ON MEASURING THE VIBRATORY PERIODS OF TUNING-FORKS

THE tuning-fork when its number of double vibrations, to and fro, in a second, or briefly its *frequency*, has been ascertained, is a most convenient instrument for measuring minute divisions of time. As such it is now extensively used for physical, physiological, and military purposes (velocity of bullets and cannon balls). The antecedent difficulty of ascertaining the frequency, is however very great. The old processes, sufficient for roughly ascertaining musical pitch, and depending upon wires of known weight, length, and tension, or the action of the siren, are totally insufficient for modern purposes. It was the contradictory nature of the results furnished by the monochord in the division of the Octave into twelve equal parts that led Scheibler to his system of a series of tuning-forks differing from one another by known numbers of vibrations, leading to countable beats, and extending over an Octave. Nothing can be more convenient to use than such a series of forks for all musical purposes. They enable the frequency not only of any small as well as large tuning-fork, but also of any sustained tone to be ascertained within one-tenth of a vibration, that is, one vibration in ten seconds. The writer has for some years been in the constant habit of using such a set of forks with the most satisfactory results. His own forks were measured by Scheibler's (exhibited in the Historic Loan Collection of Musical Instruments at the Albert Hall this year), but extend over a greater range, from about 224 to about 588 vib., that is, rather more than an Octave and a major Third. The great advantage of such a tonometer is extreme portability, immediate application to any sustained tone (even that of a pianoforte string), and the independence of the result from any (almost always imperfect) estimation of unison by a musical ear. There are of course antecedent difficulties in ascertaining the pitch of each particular fork, but these are overcome by patient observation, the extension of the series beyond an Octave furnishing in itself the required check.

Scheibler died in 1837. In 1879 Prof. Herbert MacLeod and Lieut. R. G. Clarke, R.E. (*Proc. R. Soc.*, vol. xxviii. p. 291, and *Philosoph. Trans.*, vol. clxxi. p. 1) invented an optical arrangement, which under proper management (but the manipulation was very difficult) gave excellent results for large tuning-forks, like those of Koenig. And in 1880 Koenig (*Wiedemann's Annalen*, 1880, pp. 394-417) invented a clock method for ascertaining with extreme accuracy the frequency of one large standard fork of 64 vib. at 20° C. Before both Prof. MacLeod and Dr. Koenig, Prof. Alfred Mayer, of Hoboken, New Jersey, U.S., had invented a most careful and ingenious electrographic method, of which a full account has just appeared in vol. iii. of the *Transactions* of the National Academy